ARM! For the future: adaptive resource management in the wildlife profession

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The authors encourage wildlife professionals to shift from a traditional, agricultural paradigm to an ecological one through adaptive resource management (ARM).

The wildlife profession has a long-established tradition of examining and debating the quality and direction of wildlife research (Scheffer 1976, Romesburg 1981, Bailey 1982, McCabe 1985, Capen 1989, Nudds and Morrison 1991, Lancia et al. 1993). This introspection is good, for it encourages the profession to improve and mature. In this essay, we provide what we hope will be a significant milepost in that process by advocating a general philosophy and protocol for wildlife research and management. Rather than articulating a list of specific research priorities and reiterating the need for additional research money, we encourage an encompassing, fundamental shift that will promote more efficient use of current research and management dollars.

Over the last several years, various groups and many individuals interested in the management of natural resources have recognized a need for reform in natural resources-related research. These include the Ecological Society of America's Committee for a Research Agenda for the 1990's (Lubchenco et al. 1991), the National Research Council's Committee on Forestry Research (Comm. For. Res. 1990), the Society of American Forester's Task Force on Sustaining Long-term Forest Health and Productivity (Soc. Am. For. 1993) and many others (Brussard 1991; Brussard and Ehrlich 1992; Levin 1992*a*,*b*; Levin 1993). There appears to be a general consensus that change is due.

Furthermore, intensifying political debates about management of natural resources (e.g., timber harvests and ancient forests, sustainable development, and the preservation-conservation of biodiversity) call for integrated research and management to address uncertainty in wildlife and ecosystem management, and thereby ameliorate controversy in the future (Clark 1992, Ludwig et al. 1993, Ludwig 1994). Research and management can no longer afford to be "two solitudes"; distinctions between basic and applied research have blurred (Nudds 1979, Moffatt 1994). The central issue is the application of sound scientific principles to solve problems.

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Perspective

As a profession, wildlife management is not particularly new, but it is increasingly becoming a field based on the relatively young science of ecology. Historical gains in knowledge have resulted from improved techniques, better understanding of the biology of organisms, and more experience in assessing the effects of management on particular populations, communities, and ecosystems. However, additional research is necessary to better understand how processes relate to structure in the maintenance of ecosystems (e.g., Swank and Van Lear 1992 and papers collected therein). If the diversity or abundance of all wildlife species and the environments upon which they depend are to be sustained, then we must continue to advance our knowledge of processes that underlie functioning ecosystems and the consequences of management activities (Trauger and Hall 1992). These advances in knowledge can only be made through conscious efforts to conduct sound, scientific research on the biology of wild species, their environments, and the processes that underpin species diversity and abundance in space and through time. This need is especially great because the global human population is increasing and exerting greater demand on local, regional, and global environments (Morowitz 1991).

Wildlife research: contributions, gaps, and maturation of a discipline

Aldo Leopold (1933) is credited with creating the profession of modern wildlife management in the United States with the publication *Game Management*. In the ensuing 60 years, wildlife biologists have added much to our understanding of the biology of individual species, especially those that are hunted or endangered (Trauger and Hall 1992). Comparatively less is known about nongame and nonendangered species, and within this group research has focused on large, conspicuous vertebrates (Griffith et al. 1989, Trauger and Hall 1992). We know a lot, for instance, about local habitat use (e.g., which habitats are used by a given species), we know less about local habitat selection (e.g., given a number of available habitats, which are chosen), and we know little about habitat-specific variation in fitness (e.g., how individual fitness varies across landscapes of different habitats and how such variation affects the dynamics of populations at large space and time scales). Similarly, we know little about how and why species diversity varies among habitats (Pimm and Gittleman 1992, Trauger and Hall 1992). Yet, this knowledge is crucial to making informed and justifiable management decisions about habitat protection, conservation, or enhancement (Van Horne 1983, Pulliam 1988, Mares 1992).

In some cases, patterns of habitat use have been incorporated into simple habitat-relationship models. These models characterize the habitats used by a great variety of species in various geographic regions; they can be used to predict where species are likely to be found and how management actions will affect habitat. However, such models are rarely tested for their reliability (Conroy 1993).

Wildlife biologists also have learned much about the dynamics of wildlife populations, particularly the persistence of small or declining endangered populations, the exploitation of hunted populations to achieve certain harvest goals, and the control of species that cause damage. In some cases we have been able to estimate vital rates (fecundity, survival, movement) that determine population change and have incorporated our knowledge of population dynamics into quantitative models that project population responses to different management scenarios (Conroy 1993). Again, few models have been tested; of those that have, few yield predictions of sufficient accuracy to justify their use in management (Conroy 1993). Population viability analysis has emerged as a tool to determine how large a population must be or how much habitat is needed to ensure population survival in the near future (Boyce 1993). However, much uncertainty is associated with these extinction probabilities.

Although wildlife biologists know a lot about the distribution and dynamics of populations of individual species, they know significantly less about the structure and function of communities and ecosystems, interspecific interactions such as predation and competition, and other fundamental processes such as the frequency and intensity of disturbances that shape ecosystem structure. Understanding these processes may lead to the discovery of basic ecological principles that would permit reliable predictions (Romesburg 1991).

Wildlife research has kept pace with and contributed to developing technologies used in research and management. For example, we have telemetry systems that can acquire and transmit locational, physiological, and behavioral data to computers for storage and analysis. These systems enable routine tracking via satellite of individual terrestrial or marine animals moving across vast distances. New technologies continue to be developed for spatial data collection and analysis; new methods for the genetic analysis of individuals and populations; enhancements in computing power and associated statistical and modeling capabilities; and new generations of techniques to estimate population sizes, mortality rates, and other population characteristics. Some of this new technology has stimulated data collection on large spatial scales and has vastly increased the amount of data available for analysis. Additionally, from a socioeconomic perspective, we are beginning to learn more about how humans relate to and value wildlife and other nonmarket amenity resources.

Thus, wildlife research is progressing, as any scientific discipline must, through the stages of observation and description, to theory formation. To grow, we must advance to testing theories (Ratti and Garton 1994). We can describe what animals do and where; we have accumulated an enormous array of observations and "facts"; we have incorporated this information into concepts and hypotheses about how and why the natural world behaves as we observe it to; and we have used these concepts and hypotheses to make educated guesses about the results of management actions. However, for the profession to fully mature, such information must give rise to knowledge. Our descriptions of what happens, when, and where, must lead us to better explanations for why things happen. We must develop a better predictive capability about the effects of perturbations, natural or human-caused, on biological systems (Romesburg 1981, Nudds and Morrison 1991) by testing explanatory hypotheses as rigorously as possible (Macnab 1983, Sinclair 1991, Nichols 1991). Such testing will eventually lead to improved knowledge about the structure and function of ecosystems, and management decisions then may be guided by such knowledge. Moreover from legal and ethical points of view, scientists must conduct the very best scientific analyses possible (Murphy and Noon 1991, Nichols 1991), regardless of their perspective of the role of science or how it is conducted (Murphy 1990, Drew 1994, Maddox 1994, Mosquin 1994) or whether scientists should be advocates (Decker et al. 1991, Brussard et al. 1994, Noss 1994).

A paradigm shift

Wildlife management developed implicitly, if not explicitly, in the context of an "agricultural paradigm"—one that employed simplified concepts of ecosystems in an attempt to increase yields (e.g., Lavigne 1991*a*,*b*; Nudds and Clark 1993:180). We wanted to produce abundant numbers of certain species for harvest, just as range managers wanted to grow more forage for livestock, foresters wanted to produce more fiber from trees, and fisheries managers wanted to exploit maximum yields of fish (Holt and Talbot 1978). Our scope was rooted in local issues over short time horizons. That production-consumption model usefully served our profession for a long time.

Expansion of the agricultural paradigm to an ecological paradigm must be a major priority for wildlife research and management in the future (DeGraaf and Healy 1993:24). This will allow our profession to go beyond single species management and embrace conservation of all species and maintenance of ecosystem functions (Scheffer 1976, Holt and Talbot 1978).

Just as agriculture and forestry (Jackson and Piper 1989, Espy 1993:7–8, Soc. Am. For. 1993) are realizing the need to shift toward an "ecological paradigm" that focuses on key system interrelationships of functioning ecosystems upon which sustainable resource extraction depends; so too must wildlife research and management. This does not, indeed, it cannot mean that humans will stop exploiting ecosystems. It means, simply, that we must broaden our view to include more of the "ecological services" that ecosystems provide.

Increased use of hypothetico–deductive science

To meet the challenges of the future, wildlife research must continue to make more and better use of the scientific method (Murphy 1990, Drew 1994, Ratti and Garton 1994). Much historical wildlife research has relied heavily on induction (e.g., use of repeated observations to recognize patterns and develop laws of association) and retroduction (*a posteriori* development of hypotheses to explain observed patterns), but these approaches have not led to a satisfactory accumulation of reliable knowledge (Romesburg 1981). However, substantial gains in reliable knowledge should accompany an increased use of hypothetico-deductive science in wildlife research and management (Romesburg 1981, 1991). Steps in this approach include the collection and assimilation of observations, development and specification of a hypothesis about the observations, deduction of testable predictions, development and enactment of a suitable test, and use of resulting observations to test deduced predictions (Ratti and Garton 1994).

To test hypotheses at temporal and spatial scales that are relevant to wildlife management problems and adequate for gaining reliable knowledge, wildlife researchers and managers must collaborate to take better advantage of planned management actions and manipulations (Macnab 1983, Nudds and Morrison 1991, Lancia et al. 1993). This approach can be used, for example, to test hypotheses about the presumed effects of policies regarding harvest rates, predator removal, or habitat alteration, which are of interest to managers and to practitioners of "basic" and "applied" research. These "management experiments" lie at the crossroads of "policy science" (Clark 1992) and the science of resources management; related resource management fields are also moving to embrace the concept (Loucks 1992, Walters et al. 1992).

Field and analytical techniques appropriate to large-scale experiments are being developed (Walters 1986, 1993; Carpenter 1990; Walters and Holling 1990; Eberhardt and Thomas 1991; Underwood 1993, 1994). Experimental management programs are planned or underway in several geographic locations on a variety of topics of particular interest to wildlife researchers and managers (Lancia et al. 1993). Such "management as experimentation" (Macnab 1983:398) must be a high priority for the wildlife profession in the decades to come.

Adaptive resource management—a "new" union

"Adaptive Resource Management" (ARM) is an approach to management that acknowledges uncertainty and the need to learn (Walters 1986, 1993). The term "adaptive" refers to managers learning about systems as they attempt to manage them. Using system responses to update and evaluate system models reduces the uncertainty associated with future management decisions. In adaptive resource management, learning is not simply a byproduct, but is formally acknowledged as an integral objective of the management process. Thus, management addresses the dual objectives of learning and system performance. The trick is to establish a reasonable balance between the two that will lead to optimal long-term performance. Under this management approach, learning and reducing uncertainty are valued to the extent to which they contribute to improvement in long-term system performance.

The goal of the researcher is to obtain increased knowledge about how a particular system works (e.g., about population dynamics and why a population behaves as it does), whereas the goal of a manager involves some desired system response (e.g., sustained ecosystem integrity or a change in population size). Initially, it would appear that the different goals of researchers and managers might be a source of conflict that could preclude effective collaboration. However, in the presence of some uncertainty (e.g., about population dynamics and responses to management), these goals converge because progress toward desired outcomes increases when uncertainty is reduced through learning. Importantly, too, adaptive management can help to evaluate whether what is perceived as a "desired" outcome of management is still, in the light of new knowledge, desirable or even attainable. Thus, adaptive management can lead to an inspection of values and implicit assumptions that frequently underlie management policies.

From the perspective of a researcher, adaptive resource management offers the practical advantage of working with managers so that a factor of interest can be manipulated at sufficient scales, with adequate replication and statistical power, to yield reliable inferences. For example, hunting regulations can be structured so that hypotheses about density-dependent natality or mortality can be tested (Gratson et al. 1993); the resultant, improved knowledge then can be used to improve hunting regulations. The expense of these large-scale experiments cannot be borne exclusively by research budgets—the historical absence of these experiments is testimony to this fact.

Although management of natural resources often is characterized by uncertainty and conflicting information, administrators are asked frequently to choose the "best" approach. Researchers typically argue that management should not be undertaken until more is known, yet they seldom seem to agree about what is enough. Managers, on the other hand, typically respond based on their intuition and experience, and pressed to solve a problem before it worsens, contend that enough is known to proceed with management. To make matters worse, when the need for quick action is perceived, solutions may be implemented in ways that make it difficult to evaluate whether management is successful, and if not, why not. Adaptive management offers a potential solution to these dilemmas by encouraging research and management to be conducted simultaneously as one coordinated endeavor which should reduce uncertainty and improve management.

It might be argued, of course, that in some cases current wildlife management is "adaptive." The iterative setting of harvest quotas (e.g., for waterfowl species) is a kind of trial-and-error approach that may allow for recognition of errors (harvests too high or too low) and some post-hoc remedial action. However, this permits only a limited opportunity for learning about how, or even whether it is possible, to reduce uncertainty associated with setting harvest quotas, unless the rationale for harvest quotas is based on a working functional hypothesis, or a set of models, about how and why the system (waterfowl population dynamics) works (Johnson et al. 1993, Walters 1993). Without the concomitant use of predictions about system behavior and adequate investments in monitoring, it is difficult to understand how and why the system functions the way it does. When competing explanations for how the system operates are treated as hypotheses, and evaluation is an integral part of ongoing management programs, then learning about the system is accelerated. It should be clear too, however, that adaptive management is neither trial and error nor "muddling through," nor is it consistent with the idea that, for wildlife researchers and managers, it can be "business as usual." Indeed, to be credible, the wildlife research and management community will need to be vigilant for cases where management actions are undertaken, and justified as adaptive management, when they are not.

Administrators benefit from adaptive management because it leads to decisions that are optimal with respect to management objectives. Administrators can also benefit by funding sound management experiments (i.e., adequate controls and replications of planned management interventions) because they can gauge the effectiveness of various management scenarios and can improve understanding of why a particular action succeeds or fails. Adaptive programs include periodic adjustments to make full use of new information. In uncertain environments, management decisions carry some risk, especially if one management option precludes future options. Adaptive management permits administrators and managers to hedge their bets because they are not committed to a single model (and corresponding management strategy) but can consider several simultaneously. Costly problems, unforeseen when management is initiated, may be discovered and rectified early in the process. Thus, if researchers and managers collaborate, the additional short-term costs of establishing an adaptive management program become an integral part of the cost of sound management and should be recouped over the long run.

Effective implementation of adaptive management programs and related management experiments will require some wildlife researchers to change traditional views of their roles. Researchers may have to accept some compromise and constraints in designs for large-scale experiments (Nichols 1991, Pimm 1993). Ideally, *a priori* power analyses, randomization of management treatments, and replication should characterize planned management manipulations (Schmiegelow and Hannon 1993). More realistically, however, replications and randomization will be constrained to some degree. Frequently adaptive management will be based on ongoing management programs. Regardless, what will be more important than the particular kind of investigation performed will be the attempt to learn, by testing competing hypotheses, how and why wildlife systems behave as they do (Sinclair 1991).

Researchers, to be credible, will also need to examine carefully their roles as advocates on various sides of wildlife issues in the political arena (Brussard et al. 1994). Scientists can never be sure that their analyses are not compromised by their personal experiences and values (Decker et al. 1991). Wildlife scientists will continue to be intensely scrutinized by an increasingly educated public (Murphy and Noon 1991), so our science must be objective and above reproach, especially when the best, reliable knowledge conflicts with our own values. Adopting a rigorous, hypothetico-deductive approach wherever possible should minimize the influence of personal values and biases on the results of wildlife research (Romesburg 1981, Murphy 1990).

Finally, managers as well as researchers will have to adjust their approach, especially in areas of planning and implementing management programs, because adaptive management is a process of hypothesizing how ecosystems work, monitoring results, comparing them to expectations, and modifying management to better achieve objectives through improved understanding of ecological processes (Hanley 1994). Managers should look for opportunities for replication and randomization. They will have to accept some investment of time to allow implementation of adaptive management. Likewise, administrators must provide leadership that will encourage role adjustments for both managers and researchers. These shifts are essential for science and policy to mature (Hanley 1994).

Research priorities

We do not identify specific research topics for the future because there are simply too many important questions to ask, and priorities will change continuously through time and across different regions. Rather, we suggest a fundamental shift in the way all wildlife biologists, including researchers, managers, administrators, and academicians, perceive and conduct research and management. Concomitant with this shift is the need to concentrate more research effort on revealing and clarifying basic, fundamental biological and ecological principles; without them, management of natural resources will contain an unacceptably large amount of uncertainty. Thus, the bridge between applied and basic science needs to be widened for more intellectual traffic in both directions (Romesburg 1991). However, we also must retain a firm commitment to traditional strengths of applied management which requires understanding species-population management, as well as the mechanisms affecting those populations and expanding the knowledge base to support resource-utilization programs.

As an initial step across the basic-applied science bridge, we suggest forging partnerships with other professional societies with ecological and conservation interests. For example, the Ecological Society of America's *Sustainable Biosphere Initiative* (Lubchenco et al. 1991) identifies 12 research priorities. Of these, the following are of particular concern to wildlife researchers: determine the impact of changes in land- and water-use on global and regional processes; accelerate research on the biology of rare and declining species; determine patterns and indicators of the responses of ecological systems to stress; provide guidelines and techniques for the restoration of ecological systems; develop and apply ecological theory to the management of ecological systems; develop ecological understanding of introduced species; apply ecological theory to the management of infectious diseases; and develop interdisciplinary and multidisciplinary approaches that integrate ecology, economics, and other social sciences.

In summary, we encourage the wildlife profession to shift from an agricultural to an ecological paradigm as a philosophical foundation for conducting research and management of wildlife. The Wildlife Society is uniquely positioned to lead this transition, and we strongly encourage it to do so. Furthermore, we encourage our profession to promote use of the hypothetico-deductive method to formulate and test explanatory hypotheses as a means of acquiring reliable knowledge about effects of perturbations on wildlife; to employ adaptive resource management, wherever appropriate, as a means of making optimal management decisions and reducing uncertainty; and to collaborate with other professional societies to address fundamental questions about ecological aspects of global change, ecology, and conservation of biological diversity, and to develop strategies for sustaining ecological systems that support wildlife.

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In summer of 1992, Alan Wentz, who was TWS President at the time, established an ad hoc technical review committee on wildlife research. Members representing state agencies (***Clait E. Braun and Wayne R. Porath***), federal agencies (***Michael W. Collopy, John G. Kie, Clifford J. Martinka, James D. Nichols, and Nancy G. Tilghman***), and academia (***Raymond D. Dueser, Thomas D. Nudds, and Richard A. Lancia***) comprised the committee. President Wentz charged them with documenting "... the contribution of wildlife research toward resource stewardship, identify future needs, and recommend wildlife research priorities to meet future wildlife resource conservation challenges" with the intent that TWS Council would consider the report for a technical review publication. Rather than simply recapitulating past research successes and shortcomings, the committee chose to pursue a more philosophical approach intended to help shape the future of wildlife research and management. Several drafts were submitted to TWS Council over the succeeding years. Council chose not to adopt the committee's report, but suggested that it be submitted as an essay to the *Bulletin*. The version published here is a slight revision of the original report to Council. Dr. Richard A. Lancia, Professor of Forestry and Zoology at North Carolina State University, was chairman of the ad hoc committee and therefore is the senior author.